



The Authors Reply

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Received: 28 January 2022 / Accepted: 30 January 2022 / Published online: 11 March 2022

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We welcome the confirmation by Yang and Waldhoer of our results [1] using data from Austria. The authors go on to make three statements, which we agree with. One is that birthweight without regard to gestational age is a strong predictor of risk. This of course is true, due to confounding by gestational age. After stratifying by gestational age, birthweight becomes a remarkably feeble predictor of risk. (We apologize if there is any confusion over our occasional shorthand use of “birthweight” for “gestational-age-stratified birthweight.”) It is worth noting that, while birthweight is correlated with gestational age, there is no birthweight criterion that effectively defines preterm delivery. Defining “prematurity” as less than 2500 g was a confusion that polluted clinical and epidemiologic research for decades [2].

The authors’ second point is that ROC analysis provides two options for determining an “optimum” cut point. We chose one and, as the authors demonstrate, the other leads to virtually the same conclusion.

Their third statement is that dichotomizing a non-linear relationship (a frequent strategy in clinical medicine) can be problematic. In the case of neonatal mortality across gestational-age-stratified birthweights, the pattern is clearly

non-linear, as we were careful to present (Fig. 1 [1]). Even so, dichotomies such as SGA (and preterm delivery) are so widely used as to deserve our attention. ROC analysis convincingly shows that, as a dichotomous predictor, birth before 37 weeks is useful while SGA—by any definition—is not.

Declaration

Conflict of interest The authors have no conflict of interest to declare.

References

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